International Macro and Trade Assignment 4

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Referee Report for *The Effect of the U.S.-China Trade War on U.S. Investment* (2020) by Mary Amiti, Sang Hoon Kong, and David Weinstein

Summary

The paper by Amiti, Kong, and Weinstein (AKW) aims to quantify the effect of the US-China trade policy on investment rates among US firms. To estimate this quantity, they first use a factor model / event study framework capturing the change in firms' share prices caused by specific trade policy announcements between 2018 and 2020. Using Google trends data on searches for "trade war", the authors systematically identify dates corresponding to important trade policy announcements, which they categorize into U.S. announcements, Chinese retaliation, or unrelated trade policy news. They then use 7-day windows around these events to identify the amount of share price change in the stock market that can be explained by factors common to all firms versus differential firm-type effects driven by the event. The authors find differential effects by decomposing the residuals ("abnormal returns") from the factor model further to identify how exposure to the Chinese economy affects abnormal share price returns around the policy window. The authors then connect their rather atheoretic factor model to the q-theory of investment, deriving a relationship between changes in investment and the "market-to-book" (MTB) ratio for each firm. Here, the abnormal return quantities estimated from the factor model serve as instruments that affect investment *only through* the MTB values of firms. The authors find that U.S. tariff announcements had the largest effects on share prices. Further, they find that the announcements significantly reduced firm-level investment rates, in line with the predictions of the q-theory model. The paper makes an important contribution by bridging a gap between empirics and theory, using a clever decomposition of effects identified through an event study in order to validate the predictions of a theoretical model of investment adjustment.

Suggestion 1

One area where the AKW paper could be improved is in its discussion of the explicit and implicit assumptions they make in Section 2.3. One of these assumptions, which they do mention in the text, drives the result from Hayashi (1982) that they appeal to in order to derive a tractable relationship between investment and average MTB. Investment theory models often give us a relationship between investment and marginal q (or MTB in the AKW context), but observed data only reveals average q. Hayashi derives conditions under which the two are equivalent—namely, perfect competition, constant returns to scale, and an adjustment cost function that is homogeneous of degree 1. It would be useful for the authors to discuss the validity of these assumptions in the context of the firms they study. What are the implications if these assumptions fail? Arguably it would mean that their observed MTB values are not appropriate to estimate their main equation of interest (Equation 15). Given the importance of these assumptions to model identification, they merit more discussion or justification.

Another assumption that is important to justify is the one AKW implicitly make to move from the expression in Equation 9 to the one in Equation 12. By plugging Equation 11 into Equation 9, AKW derive Equation 12,

which only includes terms that capture the impact of an event (announcement) on investment rates. In the final equation, the term p_s (the price of investment goods) is noticeably absent. The implicit assumption is that the events in question do not affect the price of investment goods at all. This seems rather strange since the price of a capital good is often written as the present discounted value of future rental rates (adjusted by depreciation and interest rates). Events such as policy announcements may affect these expectations, and thus affect prices and investment rates. If there is a different reason why the p_s term drops out of Equation 12, the authors should address it. Otherwise, the authors may be omitting an important channel of effects.

Suggestion 2

The authors should also consider adding additional robustness checks about whether anticipation of policies affect their estimates. The authors do acknowledge this as a limitation of their paper, but because it affects their estimates in myriad ways, I think it warrants more discussion. Let us first consider their factor model. They run a regression on the residuals from the factor model in order to identify the differential effect on share prices from firms exposed to China during the event window. However, it is fairly easy to imagine a story in which exposed firms also *expected* that there would be tariffs since there is a decently strong connection between policymakers and lobbyists. As a result, the differential effect of China-exposed firms would be understated since these firms would have prepared for the tariff even before the event window. Alternatively, different policy announcements could have specific effects for certain industries. As an example, AKW discuss a steel tariff as one of the events from 2018. This event may have been differentially anticipated by firms in the steel industry compared to those in other industries. In fact, AKW's finding that there is no significant relationship between stock return and protected industries might precisely fit into this kind of story. This could also affect the estimation of the IV specification in Equation 15. Further, expectations about the future based on these policies may affect the investment decisions of firms. One exercise the authors could do to address the potential anticipation of policies is to consider event windows that extend further into the past than only 1 day. It is entirely plausible that Google trends data and policy announcements align with when the "general public" find out about trade policy, but insiders know this information earlier. Extending event windows backwards in time might be a valuable robustness check to investigate how much of a role expectations play. Alternatively, a qualitative justification of why expectations do not affect estimates much would suffice.

Suggestion 3

A large concern for me in this paper springs from the specification checks and placebo tests that the authors do. As an example, AKW use trade announcements unrelated to US-China bilateral trade as a placebo test to see whether firms with higher Chinese market exposure still show significant differential effects on share prices during the event window. Yet the results from these placebo tests, presented in Figure 2, are confusing. The authors explain that when there are US or China tariff events, the distributions of cumulative abnormal returns (CARs) are left-shifted for exposed firms relative to non-exposed firms. It also seems that the distribution of CARs for exposed firms is more dispersed for other trade war events. The authors claim that there is no clear pattern of underperformance for these other events. However, given that they do not outline a metric for "underperformance" this is a strange claim. A test of distributional differences (Kolmogorov-Smirnov, e.g.) might be useful here. Alternatively, a stronger placebo test variation might be to look at how exposed and unexposed firms behave on a day with no trade news whatsoever. This would allow the readers to establish a better baseline of what the exposed and unexposed CAR distributions look like in the absence of a relevant event.

A related concern with model specification comes from AKW's presentation of factor model results in Tables 4 and 5. One of the US tariff events AKW consider is a $200 billion announcement of tariffs on imports from China. Yet, in Table 4 the authors report an effect on China Importer firms that is statistically indistinguishable from zero. Similarly, they find that Chinese tariff announcements largely do not have a

significant effect on China Exporter firms. These are confusing results unless they are further evidence that firm expectations matter (see Suggestion 2). There should be more discussion about why the empirical results seem counterintuitive to what we would expect as the policy response. Here I suggest one possible channel to explore. The authors' preferred decomposition includes the differential effect: $r^D_{ft} \equiv \sum_{j \in \Omega^U C} \sum_{i=1}^{N} \hat{\gamma}_{ij} Z_{if} D^w_{jt}$. In the vector of exposure characteristics, Z, they include only importer and exporter dummies as well as China Revenue Share. However, it is feasible that to better estimate the differential effects by firm type, there need to be interactions (e.g. between importer and exporter dummies). The authors discuss issues with their non-interacted specification, mentioning that some of the exporter effect might be picked up by the China Revenue Share variable. They address this by dropping that variable altogether in Section 4.1.1. An alternative check would be to add a fully saturated set of variables including all interactions to better capture the ways that exposure affects abnormal responses in share prices.

Suggestion 4

The authors mention that they use annual aggregates to estimate the q-theory results because a lot of previous work has used data at the yearly level. This implies that firms make investment decisions only a full year after observing economy-wide outcomes and MTB values. However, this time scale does not seem realistic. It seems reasonable that firms are making investment decisions on a quarterly basis and estimating the model more frequently would make use of the richer data that AKW have access to. Though I understand the need to be consistent with other literature, it would be useful to see whether the q-theory predictions are validated on a more frequent time scale. If the authors still find firm responses that are delayed by 4 quarters, it would be interesting to investigate why these lags persist in firm decision-making. Alternatively, the authors should provide a stronger justification for the annual time scale than just consistency with past literature.

Smaller Suggestions

A few other areas that could be expanded upon are:

- It would be good to discuss how Covid-19 might affect the late 2019 and 2020 events. Since the virus had started spreading in China during this period and eventually made its way to the US, it might influence the authors' estimation of "common" share price effects (note: differential effects are likely not as affected since the authors convincingly argue for the windows around trade policy events identified by Google trends).
- The authors say they choose a 7-day window to have the most "conservative" estimate of the trade war on US firms. However, they are not necessarily being conservative in the sense of choosing estimates that have the widest confidence intervals. There are various possible causes of bias (e.g. understating effects by ignoring expectations, measurement error if the IV assumptions fail, etc.) and it is not entirely clear that estimates are always biased in the same direction. Appealing to arguments about stock market overreaction in the short run and contamination from external events in the long run is a more appealing approach than trying to choose the window that results in the lowest magnitude of effect.
- A more thorough discussion of the IV assumptions needed to estimate the main q-theory specification would be useful. By this point in the paper, there are several layers of interdependent equations, so understanding the IV assumptions for their constructed instruments is not straightforward.